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shut out, for the nonce, the righteous demand of other interests and the cry of other needs for satisfaction. Yet unselfishness is not the exclusion of other needs and interest, it is their prosecution and fulfilment. Unselfishness is not the repose of one's own perfect adaptation to the environment; it is the unrest which compasses that adaptation for others. In the experience where beauty is the relation between you and your environment, it is, however, you yourself who are so adapted, and, being adapted, lifted up and out of the horde of conflicting interests. Your world is that object to which you are bound, and you are become isolated, alone, and supremely happy in that loneliness. Here is the only genuine solipsism, in which the stuff of reality assumes the status of mentality and things and thoughts are one. It is of the essential nature of beauty that your neighbor can have no part in your experience of its object, and that your experience of it can have no part as such in any other concern whatever in the enterprise of life.

Private, concrete, elusive, in itself neither mental nor amental, beauty is the optimal mode of that positive, intrinsic value-relation which binds the mind to its object in such wise that the two are completely and harmoniously adapted to each other in the very act of apprehension.

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IMITATION AND ANIMAL BEHAVIOR[1]

ADVANCE in the experimental analysis of behavior tends to make psychological concepts inadequate. In the realm of human psychology one needs only to instance such a term as memory. Aristotle summed up his total discussion of this subject in sixty words. With modern psychology came experimental analysis and to-day it requires twice sixty words to name the separate subjects that we investigate in the general field of memory. It would be an easy matter to show the same analytic tendency in perception and thought and will and in many non-psychological fields as well. It would be no less easy to point out numerous fields where such analysis has not had its way, and comparative psychology is one of these. It does not require any great insight in the reader of comparative psychology to see that many of the concepts used in the description of animal behavior are of the relatively unanalyzed sort. That we continue to talk in general about growth, development, intelligence, instinct, and imitation is evidence only of the fact that we have not yet pushed our experimental analysis to the end—not far enough to see what in reality are the elemental processes out of which the complex behavior of animals is built up. I insist on the phrase "experimental analysis," for it is only by the most extensive and painstaking development of detailed methods and the application of these methods in quantitative studies that we shall ever be able to understand animal behavior and to see its intimate relation to human behavior.

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[1] Read at the twentieth meeting of the American Psychological Association, Washington, D. C., December, 1911.

Take the case of imitation. There can be no doubt that the facts which this concept has been used to connote are more complex than any writer has yet set forth. It was no doubt a distinct advance in the discussion of the subject when scientists distinguished instinctive from voluntary imitation. This, however, is not a finally satisfactory analysis of the concept, and one reason why we have not made more progress in our study of the imitative behavior of animals is that the whole subject has been dominated by this crude differentiation. We have been looking for something that could be called instinctive imitation or voluntary imitation, and the facts have not fit this division. It would probably be more correct to say that psychologists have been looking for a sort of animal behavior that could be called voluntary imitation, and when they have found imitation that did not fulfill their idea of what constituted volition or inference they have gotten rid of such imitative behavior by calling it instinctive. The results of such study have not been encouraging, and experimentalists have tended to turn away from the study of imitation to fields that promised more definite results.

Before this diversion from the study of imitative behavior is complete it may be worth while to examine the tools with which we have been working. After all what can one mean by instinctive imitation? Whatever he means by imitation, it must be qualified by what he means by instinct. And what does instinct mean in current psychological discussion? If one is content with verbiage, he may, after perusing a whole library on the subject, as Wheeler admits doing, and exercising the most arbitrary selection, satisfy himself with a form of words. If he is not a word-monger and insists on knowing concretely what instinct means in the analyzed behavior of any single mammal, there is scarcely a line in the experimental literature, except Yerkes's and Bloomfield's[2] work on the cat, to illuminate him.

Let us try to be concrete. Speaking from the point of view of current thought, we would doubtless all agree that there is in the young of mammals an instinct to hunt out the breast and suck.

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[2] "Do Kittens Instinctively Kill Mice," *Psych. Bull.*, 7: 253.

Now take the case of newly-born puppies and ask any single question about the makeup of the instinct; ask what it is that sets this instinct going, and you will not find a satisfactory answer anywhere in the literature. That it can be neither sight nor sound seems evident, because the eyes and ears of new-born puppies are closed for practically a fortnight after birth. Yet it is an open question whether they can not distinguish shades of light through the closed lids. Suppose you eliminate light and sound. What do you know about the puppy's sense of smell, its power of discrimination, its range in quality, and its range in intensity; the exceptional power of certain odors to excite reaction, the distance over which the odor is perceptible, the power of localization? To every one of these questions you must answer, "Absolutely nothing specific." What about the new-born puppy's sense of temperature, its sense of touch, its power of orientation, its possible kinesthetic sensations, its oral sense, its ability to taste? To every one of these interrogatories you must reply as before, "Nothing at all that fulfills the demands of experimental science."

If you seek to know which of several stimuli is prepotent over the others and to determine some order of importance for the several possible senses, you complicate the situation still more, and your confusion increases if you raise the question of the relative accuracy, serviceableness, and modifiability of the supposedly connate neural connections. It is hardly necessary to do more than state the situation to see that when we speak of the feeding instinct of young mammals we are merely cloaking our ignorance with a phrase. As an analytic concept it is valueless. Yet, if we have so little knowledge of the first experiences of the new-born animal, all its later history is clouded in even denser mists. There have been some studies on the sense of hearing and the sense of sight in dogs, but this work is not sufficiently accurate in its technique but that later experimentalists will insist on doing it all over again. There has been some work on dog intelligence, but not one of the reported investigations has even attempted to take the dog on his own ground, that of smell, and in no one of the investigations has the experimenter succeeded in eliminating himself from the experimental situation. These two shortcomings very decidedly limit the value of any investigation as yet made. When you couple with the evident fragmentariness of the experimental work and its certain lack of finality, the fact that the behavior of a dog at any level of development is a composite of inherited and learned reactions, you see how impossible it is in any given case of canine behavior to say what is instinct and what is intelligence. Gross facts are evident enough, but we ought at this time to be beyond the stage where we base theories of learning on the simple observations of common sense.

Yet, in spite of our inability in concrete cases to separate instinct from intelligence, we are asked by current writers to regard a large proportion of dog behavior as due to instinctive imitation. I confess that I can not see how this sort of speculation is likely to illuminate the subject of animal behavior. To use the phrase to point out a large body of unanalyzed behavior is of course allowable, on condition that we take the next imperative step in the process, namely, to analyze that behavior into its elemental terms. But to imagine that we have said something final about a certain bit of behavior when we call it instinctive imitation is to mislead ourselves and to confuse the rightful course of experimental investigation.

With voluntary imitation the case is even worse. In human psychology we are at sea as to what constitute the elemental processes of inference and volition. In one place we read that the highest processes of mental life are nothing more than highly elaborated complexes of functioning images. In another place we are told that all this image-mongering is absurd, and that volition and inference can go on without any images whatever. On the one hand, we hear that we are nearing the end of sensationalism, and on the other, that the final triumph of sensationalistic psychology is even now in sight. Then we hear that there is no valid objective criterion of the presence of imagery—that we must always depend upon the subject's introspective report. In the light of such confusion, such a term as voluntary or inferential imitation loses its significance. Until human psychology can give us something more settled regarding the processes of volition we do well to use the term volition with parsimony in reference to the doings of animals.

Here then is our situation. We have the concept of imitation, which is an essentially descriptive term, setting forth certain features in the objectively observable behavior of animals. This concept is then divided into two parts, not, mark you, on the basis of objectively observed features of behavior, but on the basis of the supposed psychical accompaniments of such behavior. The terms which are used to denote these two divisions then become, not descriptive terms any longer, but explanatory terms, *i. e.,* they do not point out the behavior which actually takes place, but they attempt to indicate the non-observed processes antecedent to such behavior. These terms, however, when submitted to critical examination, turn out to have the most uncertain significance, for, imitation entirely apart, it must be admitted that there is no understanding about the relation of instinct and volition. What is more is that we shall not have any understanding of their relation so long as we confine our work to the logical differentiation of terms. It may not be a very encouraging situation, but there is little likelihood that anybody will say anything significant and concrete about instinct and volition in mammalian behavior until we have a far larger accumulation of experimentally determined facts than we now have regarding any single mammal.

This situation is an unfortunate one for the study of imitative behavior, which is no longer approached on its own merits, but which has to struggle for recognition under the burden of supposedly explanatory adjectives, which in fact explain nothing, being themselves in need of description and explanation. We seem to face two alternatives: we may abandon the study of imitation and direct our studies to other fields. This we seem to be doing and to a degree the tendency is commendable. If the change is actuated by the feeling that imitative phenomena are so complex that we can not rightly interpret the results of experimental studies on imitation until we know more about the sensations and instincts, then, I agree. If, however, the tendency to drop imitation out of our categories is due to the belief that when we are talking about imitation we are resorting to "magical agencies" and that we must abandon it in favor of something that is more truly scientific, then I dissent, and insist that whatever may finally be our decision regarding imitative phenomena, we are as yet without sufficient evidence for any such speedy termination of this category. No person can face the whole group of experimentally determined facts of imitation in birds,[3] rats,[4] cats,[5] monkeys,[6] and apes[7] and come to any such conclusion, except he do it in behalf of a theory which he regards as more important than the facts.

The second alternative is to suspend judgment as to the particular level of psychical accomplishment denoted by the different kinds of imitative behavior, to free the concept of imitation from its unfortunate appendages and set ourselves to the task of accumulating the facts which we shall need before we can finally determine the importance of any particular kind of imitative behavior. The social relations of animals are of vast importance to their degree of mental attainment, and in these social relations there is a kind of behavior, which, to date, has not been better described than to call it imitation. At the present stage of our study of these relations, it is of secondary importance whether we are finally to explain them as "inherited reactions which are definitely serviceable on the occasion of their first appearance," or whether we must group them under entirely new rubrics. It is of first importance that we find out in terms of objectively describable behavior exactly what these relations are, and find it out in elemental terms.

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[3] James P. Porter, "Intelligence and Imitation in Birds," *Amer. Jour. Psych.*, 21.

[4] Charles S. Berry, "The Imitative Tendencies of White Rats," *Jour. Comp. Neur. and Psych.*, 16: 333.

[5] Charles S. Berry, "An Experimental Study of Imitation in Cats," *Jour. Comp. Neur. and Psych.*, 18: 1–25.

[6] M. E. Haggerty, "Imitation in Monkeys," *Jour. Comp. Neur. and Psych.*, 19: 337.

[7] M. E. Haggerty, "Preliminary Studies on Anthropoid Apes," *Psych. Bull.*, 7: 49.

I said a moment ago that no one can face the whole group of experimentally determined facts of imitation in animals and treat them lightly. I wish now to call attention to a single case of imitation which I reported to this association three years ago. Two monkeys were put into a cage three by four feet at the bottom, and six feet high. Seven strings hung from the top of this cage to within eight inches of the floor. Near the floor was a circular opening in the back of the cage, and one of the strings was attached on the outside of the cage to a mechanism which would, when the string was pulled, drop food down through a chute on the outside of the cage to a floor level with the opening in question. One of the two monkeys had learned to pull the string and get food at the opening. The other monkey, although he had been allowed ample opportunity to learn the trick unaided, had failed to do so. After being allowed to be with the first monkey when she pulled the string and got food, the second animal when left alone directed his attention to the food opening in a way that he had never done and repeatedly handled the three strings nearest the opening in a far more interested manner than he had ever done. In explanation of this change in behavior I am perfectly willing to invoke Thorndike's first law of behavior[8] that "*the same situation will, in the same animal, produce the same response—and that if the same situation produces on two occasions two different responses, the animal must have changed.*" But then I would ask those who deny that this is imitative behavior to specify in what the change in the second monkey consists. To assume that there has been a change independent of the presence of the performing animal is mere gratuity. The evidence was too clear that the attention of the stupid monkey received a decided and sudden turn in the direction of the behavior of the other animal to doubt that that behavior was the determining factor. That the second monkey should go to the opening and look in may, of course, be explained by the fact that he had seen food there, but that he should suddenly become interested in the strings, the ends of which hung six inches above the opening and out of the animal's range of vision when he was looking into the opening, can receive no such explanation. There had been ample opportunity for the second monkey to learn the trick unaided, but he had failed to do so; the strings had never brought satisfaction to him through his own activity. Yet now, although he did not use the strings to get food, he continued to handle them, to pound them against the side of the cage and against each other, and several times after acting in this way he looked directly into the food opening. Such continued interest can not be explained by the "law of effect." There is here a directing of attention that can not be due to the activities of the animal itself nor to any change in the mechanical situation.

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[8] Edw. L. Thorndike, "Animal Intelligence," page 241.

This directing of attention which is so evident in this case was more marked in the next stage of the animal's learning. The trained animal was put back into the cage and allowed to get food in the presence of the learning monkey. As a result of this experience the attention of the second animal was narrowed down to the correct string. He no longer played with all three strings but centered his attention on the correct one of the three, and that without ever having used it in getting food or finding satisfaction through it in any other way. That he did not at once do the necessary thing to get food shows that imitation was not perfect and had to be pieced out with accidental learning, but the very fact that, in spite of his inability to do the proper act, he kept working at the task shows that the law of effect is not sufficient to explain this kind of learning. I do not claim that this is voluntary or inferential imitation. I do not profess to have any very clear idea as to what voluntary and inferential imitation are. What I do claim is that you have here a progressive narrowing of one animal's attention (viewed objectively) in the direction of the behavior of another animal and that this change in the behavior of the second animal can not be accounted for by any supposed change in the animal itself, except such as is induced in it by its observation of the successful behavior of the trained monkey.

If my contentions in this case are granted it may be urged that this is an exceptional case. I doubt that. My own investigation showed other cases which can not be explained on the basis of the supposedly simpler laws. To be sure I do not claim any finality for my results. The investigation marks only one stage on the road of experimental analysis and only points the way for extended investigations in the same direction. The methods of procedure will bear favorable comparison with those of any published experiments in this field, and under these circumstances I shift the burden of proof to the objectors. They must take the experimental devices which produced these results and show that under the same conditions most monkeys will not do as the ones whose behavior is reported.

In view of these contentions, to which I have tried to give some degree of reasonableness, I do not think that the time has come to discard our study of imitative behavior as Bohn[9] seems to think, nor to throw aside the category of imitation as Thorndike would have us do. That a final interpretation of the facts must wait upon the accumulation of a much larger body of material than we now have is certain. On the other hand, there is equal certainty that we must not telescope the facts so far ascertained with theories that do not give full justice to these facts. What our present situation indicates is a reworking of the concept of imitation by discarding the old classification and proceeding to a new classification based on objectively observed facts. That the experimentally determined data are as yet wholly inadequate for a final statement is admitted. Such a reorganization must take account of all the factors that determine attention and of the various levels of accuracy and complexity in the imitative behavior. The first step in the process of reorganization is to convince ourselves that the old classification has reached the limit of its usefulness; the second step is to construct a new classification for a single species of animal, and to follow this with a like service for other species, in every case basing the classification on the facts which have been brought to light by experimental investigation; the third step will be to push the experimental analysis of imitative behavior much farther than we have yet done, and in the end we may be able to speak with positive understanding about the imitative behavior of animals.

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REVIEWS AND ABSTRACTS OF LITERATURE

Motive Force and Motivation Tracks: A Research in Will Psychology. E. Boyd Barrett, S.J. London: Longmans, Green, & Company. 1911. Pp. xiv + 225.

Those who are watching the progress of psychology will easily be reminded, through the present work, of Cardinal Mercier's efforts to interest catholic philosophers in experimental psychology. Broadly speaking, the Cardinal's propaganda in favor of the latest phases of psychological research can not be said to have been very fruitful among his correligionists. Where they have tackled psychological subjects experimentally, in following Cardinal Mercier's advice, they have done so with the intention of showing the exact manner in which the catholic philosopher must look upon experimental psychology rather than for the purpose of solving any particular problem.

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[9] Georges Bohn, "La Nouvelle Psychologie Animale," page 185.